all prostrated. *Mimosæ* of various kinds, also flowering, and the more tender palms, were borne down and broken. Pelargoniums and other succulent shrubs destructively crushed. Partial thaw in the sunshine.

March 8.—Th. min. 27°·7, max. 51°·3; bar. 28·83. Sunshine in morning began a thaw, but only to discover mischief done by the frost. Wind first from N.E.; in p.m. from S.W., increasing thaw.

March 9.—Th. min. 35°, max. 51°; bar. 29·67. Rain in night and most of day, but later turned to snow in large flakes. Wind S.E.

March 10.—Th. min. 27°, max. 44°; bar. 28·88. Fresh snow in night to depth of 4 or 5 inches. Whole country white, including Esterel Mountains, on which snow is hardly ever seen. Wind W., rising, threatening a mistral. Only two small spots on the sun.

March 11.—Th. min. 24°·1, max. 45°; bar. 28·84. Bright morning, but intense cold with mistral, at night destroyed almost all tender plants and shrubs in garden, in spite of covering. One fine young indiarubber-tree of 15 feet, with its rich green and bronze leaves, turned in the night to a spectre of limp black rags. Wind W., calm. Only one small spot on S.E. border of sun.

March 12.—Th. min. 25°·7, max. 49°; bar. 28·90. Sun bright, but hard frost everywhere except in sheltered places. Wind W. strong. Four spots now visible on sun, one larger than the rest, and near it a large oval facula of brighter light.

March 13.—Th. min. 32°·1, max. 49°·6; bar. 29·30. Weather bright, wind W., moderate. Two of the four spots larger, with deeper umbræ; suspicion of a facula near one.

March 14.—Th. min. 29°, max. 54° (?); bar. 29·50. Sky bright, some haze, wind W. Four sunspots, less marked, varying from day to day; one, which was a penumbral streak, now hardly visible.

March 15.—Th. min. 32°, max. 50°·4; bar. 29·30. Weather feels much warmer, wind W.S.W.; one of the sunspots much larger, with a rent of dark umbra within.

March 16.—Th. min. 36°·7, max. 50°·3; bar. 29·19. Weather fine, a little haze, wind W.S.W. Now five spots; two large, with dark irregular centre and fringe of penumbra; two dark, without fringe; one a mere streak of penumbra.

March 17.—Th. min. 41°·9, max. 52°·2; bar. 29·22. Fine in morning, but hazy; later, clouds from S.W. (showing rain-band) gathered, and brought first hail, then rain for two or three hours; later, the sun appeared with one of the new spots much enlarged, consisting of a penumbra with two distinct dark clefts within.

March 18.—Th. min. 35°·1, max. 53°·9; bar. 29·48. Bright morning, with haze, wind S.S.W. No change in sunspots.

March 19.—Th. min. 45°·9, max. 52°·5; bar. 29·20. Morning gloomy, with clouds and rain. The wave of cold seems to have passed, but not so the vast deposits of snow left on the mountains behind, and still less the unknown detriment inflicted on vegetable life in the olive and orange groves around us.

The foregoing observations are too few and too imperfect to warrant any decided conclusions, but they add to those already made in evidence of the connection between the absence of sunspots and the diminution of terrestrial heat; and I trust they may be followed by further and more exact investigations to determine the influence of our great luminary on the weather and climate of the world. How far this "cold wave" has extended to other countries and latitudes I am not informed; but it seems to me that their usually cloudless skies bring the shores of the Riviera into closer and more direct relationship with sun-power than other countries, and therefore render them more sensitive to its variations.

C. J. B. WILLIAMS

Cannes, March 19

Mr. Grant Allen's Article on "The Shapes of Leaves"

THE article by Mr. Grant Allen on "The Shapes of Leaves," published in NATURE (vol. xxvii. p. 439) as first of a series, calls for an emphatic protest on behalf of botanists, and especially of teachers of botany.

In his introductory paragraphs he at once cuts the Gordian knot of vegetable physiology in a most startling manner. He tells us that "from the free carbon thus obtained [*i.e.* by deoxidation of carbonic acid], together with the hydrogen liberated from the water in the sap, the plant manufactures the hydrocarbons which form the mass of its various tissues." If he had only substituted, by a slip of the pen, the term hydrocarbon for carbohydrate, it might have been regarded as a pardonable piece of negligence; but, since he speaks of "*free carbon*" and *hydrogen*, he shows that he really meant to write the word "hydrocarbons." Naturally he does not bring forward the results of any experiments which may have led him to make this extraordinary statement.

He goes on to say: "Vegetal life in the true or green plants consists merely in such deoxidation of carbonic acid and water, and arrangement of their atoms in new forms." Among other strange conclusions to be drawn from the above lines we see that, according to Mr. Grant Allen, either nitrogen does not enter into the composition of proteids, or that the latter have nothing to do with that "vegetal life" of which he speaks.

Articles containing blunders of such magnitude, but written with that assurance of style which naturally carries conviction to the mind of the unwary, and disseminated through the country in a widely read journal like NATURE, cannot but produce a rich crop of erroneous impressions. These it will be the arduous duty of teachers to eradicate.

Every one will agree that the popular writer must, before all things, be master at least of the first rudiments of the subject on which he writes: Mr. Grant Allen has in two consecutive sentences shown himself singularly deficient in this respect.

It would be premature here to enter upon a detailed criticism of these articles, since the series is not yet complete. But the two sentences I have quoted are so strangely heterodox that they could not be passed over without remark.

F. O. BOWER

As I do not think it necessary to preface four short papers on the shapes of leaves with a formal treatise on physiological botany, I am not careful to answer Mr. Bower in this matter. The word hydrocarbons was used deliberately, because the important point to notice is this—that the plant consists in the main of relatively deoxidised materials. From the point of view of energy, with which one has to deal mainly in treating of functions of leaves, that fact is of capital importance. I can conscientiously inform Mr. Bower that I was aware of the chemical constitution of proteids, and of the part which they bear in life generally; but I do not see what harm can be done to anybody by such a confessedly rough statement as that which he criticises. If we must always step aside to say all that we know about any subject whenever we have to deal with it, exposition of new matter becomes impossible. May I call Mr. Bower's attention to the further fact that in the same paper I spoke of the plant catching "fragments of carbon," meaning thereby not free carbon, but carbon in the form of carbonic acid, even though it be merely reduced from carbon dioxide to carbon oxide. It seems to me that such roughly accurate language is permissible in popular writing, where one's main object is to insist only on the general principle involved. It is the carbon that the leaf wants, not the oxygen; it is the carbon and the hydrogen that it deals with, not the nitrogen, which is but the instrument for dealing with them; and the two other elements may therefore be safely neglected. Or must we drag in sulphur, and potassium, and calcium, and all the rest as well?

GRANT ALLEN

Ticks

IF W. E. L. will acquaint himself with the somewhat scattered literature of this subject he will find that much useful information has already been placed on record by entomologists and others. The *Farm Journal* for July 10, 1880, contains a sensible and convincing article by Mr. James Elliot, showing the connection between ticks and louping-ill. A good article on the sheep-tick (falsely so called, since it is an insect and not one of the Ixodidæ) occurs in *The Field* for April 26, 1873. The scientific aspects of the subject are well treated of by Mégnin, especially in relation to classification in his "Monographie de la Tribu des Sarcoptides Psoriques," 1877. Mr. Hulme's edition of Moquin-Tandon's "Elements of Medical Zoology" has a useful chapter on ticks (p. 302). Some valuable hints are given in Prof. Verrill's Report on parasites to the Connecticut Board of Agriculture, 1870. An excellent article with good figures on *Melophagus ovinus* appeared in one of the volumes of the *Intellectual Observer*. The ticks of the sheep and stag are both figured in Van Beneden's "Animal Parasites" (English edition of International Series, p. 177). The sheep-tick is likewise figured and described in the "Micrographic Dictionary." References and

figures are also given in the standard works of Westwood and Packard on insects. As W. E. L. is probably a practical man, he will do well to consider the proofs afforded by Mr. Elliot that the "ked," as they call it in Scotland, is anything but the harmless insect which some people imagine it to be.

T. SPENCER COBBOLD

I AM inclined to think your correspondent W. E. L., on the subject of "ticks" (p. 531), may have confounded two quite distinct animal forms under that name. The sheep-tick or louse, as shepherds call it, found at the roots of the wool on sheep, and which I have often formerly had brought to me under one of those names, is an aberrant form of *Hippobosca*, a genus of dipterous insects, the typical species being the well-known forest-fly. An excellent figure of the sheep tick will be found in Curtis's "British Entomology," Pl. 142, under the name of *Melophagus ovinus*.

Ixodes is a genus of the Acaridæ, a group easily distinguished from the true insects by their having eight legs in the adult state. Six British species of *Ixodes* are described by Dr. Leach in vol. xi. of the *Linnean Transactions*. There are probably others not as yet determined. The one best known is the common dog-tick, found in a free state in woods and plantations, and attaching itself not merely to dogs but to hares, &c., and especially to hedgehogs, which often abound with them, the ticks getting their hold as the animals pass through the close grass. After attachment they soon get gorged with blood, their abdomens swelling to an immense size compared with the insignificant appearance of them previous to attachment. But I can remember no instance of an Ixodes found on a sheep, though I would not undertake to say they never occur on that animal.

Bath L. BLOMEFIELD

Helix pomatia, L.

I AGREE with Mr. Gwyn Jeffreys (NATURE, p. 511) in considering *Helix pomatia* as indigenous in this country, and not introduced by the Romans. I never found or heard of a single specimen, either living or dead shell, being met with in the neighbourhood of Bath, which the Romans occupied for more than 400 years, though it is found in one or two localities in the adjoining county of Gloucestershire, from whence we have specimens in the museum of the Bath Literary Institution.

Bath L. BLOMEFIELD

Braces or Waistband?

HAVING worn a Spanish sash for some time many years ago while walking in the Pyrenees, I am decidedly of opinion that the weight of the trousers is supported much more easily and pleasantly by a sash than by braces; these last are narrow, about 2 inches wide, and though custom enables us to wear them without conscious inconvenience, I think any one using them for the first time would find them very unpleasant. The sash worn by the middle and lower class in Aragon is of wool 8 or 9 inches broad, and (if my recollection is correct) about 4½ feet long; when of such width and length it does not need to be drawn *tight*, but only closely wrapped round the waist and the end tucked in. I should certainly wear one constantly but that I do not wish to have an eccentric appearance. Medical men, I believe, attach great value to the wearing of sashes or bands round the stomach, especially in hot countries. A narrow silken sash which must be drawn tight is, I should suppose, far less pleasant to wear.

N.

SOLAR RADIATION AND GLACIER MOTION

IN the paper on the "Mechanics of Glaciers," which the author had the honour to read before the Geological Society of London in December last, it is stated that, after all allowance is made for work within the glacier due to the potential energy of the *weight* of the ice-mass, "there remains to be accounted for a *secondary* differential motion, which has, it appears, not yet received a satisfactory explanation . . . the movement is greater (*a*) by day than by night, (*b*) in summer than in winter." The present paper is intended as nothing more than a brief statement of the experimental evidence, upon the strength of which the explanation offered in the paper referred to has been put forward. I may say *en passant* that this investigation was suggested to me by a statement of Dr. Croll's ("Climate and Time," p. 519) that, "We find that the heat applied to one side of a piece of ice will affect the thermal pile on the opposite side." It occurred to me that the looseness of this statement was quite in keeping with the *un*physical notions upon which the writer has built up what he styles his "molecular theory" of glacier motion, and I set to work therefore to investigate its accuracy.

The principal apparatus used consisted of a delicate galvanometer, and a thermopile of a pretty high degree of sensitiveness, made up as it is of eighty-one couples of bismuth and antimony; the measurements were read off numerically by the light reflected on the scale as usual. Suspecting that the fallacy of the statement referred to lay in overlooking the effect of luminous energy, which of course is capable of passing through any *transparent* body, I made a few preliminary trials with glass and water, not having ice then at hand. A beam of solar radiation, having passed through two inches of distilled cold water + half an inch of glass, was allowed to fall upon a Crookes' radiometer; this made the vanes rotate too fast for their rotations to be counted, even when the instrument was enclosed in a wooden case on all sides except that open to the glass-water screen through which the sunshine passed. A beam of solar light, having been sifted of its dark heat-rays in the same manner as before, was received upon the absorbing face of the thermopile, producing a considerable deflection of the magnet in the galvanometer, even with the feeble sunshine of our recent December days.

The next step was a series of trials with *ice* itself. In the first instance, trials were made with the plates of *ice in contact with the metallic face of the pile*, the black (absorbing) face being placed at a distance of 3 inches opposite a large Bunsen flame in a room free froms trong draughts: in this way a constant difference of 36° C. was obtained for the opposite faces of the pile, and maintained for more than half an hour, with the needle of the galvanometer quite stationary. An iron ball 3 inches in diameter, having been heated to dull redness (clearly perceptible in a dark room), was placed opposite the plate of ice (1 inch thick) in contact with the pile, and allowed to cool. It was again heated as before, and placed at a distance of *less than an inch* from the ice (now less than half an inch thick), and allowed to cool. In both cases the effect observed upon the galvanometer was *absolute nil*, even when, in the second trial, the ice had become so thin by melting as to break under the small force required to hold it against the pile.

In the next series of trials the arrangement was reversed, the *ice* being placed just in front of the condensing cone attached to the absorbing face of the pile at a distance of 4 inches; the metallic face of the instrument was maintained at a constant temperature by contact with a vessel of cold water, whose temperature was observed frequently, and found to be practically constant. On the distant side of the ice was placed a double board-screen, with air-space and a circular hole to allow the passage of a cylindrical beam of radiation of the same diameter as the condensing cone. The iron ball, heated to dull red heat as before, was placed opposite the hole of the screen, at a distance of 7½ inches from the face of the pile, the intervening ice-plate in this case being 1 inch thick, and the galvanometer having been stationary for half-an-hour before the experiment was made. Under the same conditions the experiment was repeated (1) with ¼-inch plate of ice; (2) with ¼-inch of pond-ice + wet half-melted snow; (3) with ⅝-inch of fresh-fallen snow. In all these cases the result of the obscure radiation from the ball upon the galvanometer was *absolute nil*, although, without the interposition of ice or snow, the maximum